

moral justification. And to assume that it is so is to beg the whole question.

Only one other argument of an ethical kind remains to be considered, and we are sorry to say that it has been advanced by Lord Coleridge—sorry because it is so childishly weak. It is the old argument that if the advancement of knowledge is taken to justify the vivisection of animals, as much or still more should it be taken to justify the vivisection of men; and in view of the horrible possibility thus supposed, Lord Coleridge exclaims—"I hope that morals may always be too much for logic; it is permissible to express a fear that some day logic may be too much for morals." Logic! Only on the assumption that an animal is a rational and a moral as well as a sentient creature, and that its reason and its morality are on a level with those of man, would the argument become logically valid; and it is just because the physiologists *do* "consent to limit the pursuit of knowledge by considerations not scientific but moral," that they are obliged to draw the same logical distinction between men and animals as that which is drawn by the Legislature.

Coming lastly to the side of Religion, Mrs. Kingsford concludes her article with a paragraph which we think worth quoting, as it may serve to indicate the value of her opinions generally: "If I should be asked what is the real position taken by the leading champions of 'free' vivisection, and concealed from the public under the plea that the practice conduces largely to the benefit of humanity, I would define it thus:—

"1. Repudiation of the religious and sympathetic sentiments, and of the doctrine of man's moral responsibility as superstitious and untenable.

"2. Deliberate determination to dissociate themselves from all but those who join in such repudiation; and to make the practice of experimental physiology on living animals a rallying-point for the expression of that determination."

Surely it must appear to Mrs. Kingsford that these "leading champions" are adopting somewhat roundabout means to secure their very remarkable ends.

Lord Coleridge asks: "What would our Lord have said, what looks would He have bent upon a chamber filled with 'the unoffending creatures which He loves,' dying under torture deliberately and intentionally inflicted?" And Prof. Yeo answers: "I cannot imagine any such chamber of horrors, any more than I can his other hideous suggestion;" and adds that concerning the real facts of vivisection as performed in this country, "my conscience unhesitatingly tells me that it would have met with the full authority and approval of our Lord. . . . And I like to bear in mind the texts which seem to have an accurate bearing upon the subject, 'Ye are of more value than many sparrows,' 'How much then is a man better than a sheep?'" Similarly Sir W. Gull and Dr. Carpenter support physiological research on grounds of Christianity and Theism, and it is evident that the religious side of the question really hinges on the ethical. If vivisection is cruel, it is also irreligious; but if it is the highest mercy, physiologists may claim, though from those to whom their work has been of priceless value they may not always receive, the beatitude of the merciful.

FISHER'S "EARTH'S CRUST"

Physics of the Earth's Crust. By the Rev. Osmond Fisher, M.A., F.G.S. (London: Macmillan and Co., 1881.)

MR. FISHER is well known to geologists as the writer of various important papers on Mountain Chains, Terrestrial Heat, and other physical phenomena of the earth. He has in this volume not merely collected these papers, but added so much new matter that they form only a small part of the book. It deals with those regions whither we cannot penetrate, and might be called a Treatise on Concealed Geology.

It has been made a reproach to geologists that their mathematics never get beyond the Rule of Three. Mr. Fisher may redeem them from the reproach. Indeed an unmathematical reader, when he sees pages covered with symbols, may be tempted to close the book in despair and imagine it a case of *μηδεις ἀγεωμέτρητος ἐνείτω*. However he would not act wisely. If he read steadily on, only omitting such calculations as he cannot understand, he will obtain many fruitful ideas, and follow several chains of sound and careful reasoning.

After a discussion of the rate of increase of temperature met with below the surface of the earth (which he concludes by adhering to the customary view of a uniform rate) Mr. Fisher reprints his former calculations of the enormous and overwhelming pressure to which the crust of the earth would be subjected, if the interior shrank away from it by contraction. The pressure would be such as the strongest rocks could not resist. The engineers of the St. Gothard Tunnel were almost baffled in attempting to sustain less than a mile's thickness of yielding rock. What arches or rings, what metal or granite would stand two thousand times that stress? There can be no doubt therefore that contraction is a cause adequate in intensity to contort any strata however thick, or uplift any continent however lofty. Adequate in intensity most certainly; but has it been sufficient in quantity? This question Mr. Fisher next considers. The answer will probably surprise many geologists.

When the earth first formed a solid crust with a glowing nucleus reaching to within a few feet of the surface, the nucleus would begin to cool and contract. As it shrank, the shell settling down on to it must crush itself into wrinkles. As successive internal portions solidified and were united to the solid crust, the remaining nucleus would continue to shrink, and the volume crushed out from the crust in process of accommodating itself would grow correspondingly greater. The wrinkles would be magnified. From Sir W. Thomson's formulæ for the internal temperatures of a cooling globe Mr. Fisher calculates the total volume of the wrinkles that could have been produced by now. He shows that this cannot possibly be so much as the fifteenth part of the volume of continental elevations above the sea bottom: more probably not even the sixtieth part. Though he considers the nucleus fluid, while Sir W. Thomson thinks the whole globe would have been solid or nearly so, this does not seem to affect the correctness of the conclusion. At the same time this cause, inadequate for continents, might yet be abundantly sufficient for the existing mountain chains, and for many predecessors of them.

The theory of the earth's constitution put forward in this volume is that the surface on which we stand belongs to a crust some thirty miles thick, floating on a substratum of slightly greater density. Below this substratum *may* be a solid nucleus, *must* be if Sir W. Thomson's proof of the earth's rigidity be accepted; but this book does not profess to go deeper. The floating crust cannot be supposed to possess much strength, so that the weight of mountain chains would break through it, unless they have beneath them corresponding protuberances on the under side of the crust, which shall support them by the additional buoyancy so produced. A plastic crust under compression would yield above and below, and thicken, as two pieces of hot sealing-wax spread out when pressed together, so as to give rise to such double bulges as are supposed. However, it is shown that even on this theory contraction cannot have produced the whole of existing terrestrial inequalities of surface, and could hardly even have lifted the continents above the sea-level. The general result of these suppositions would be that the crust beneath the ocean basins must be denser and thinner than that beneath the continents. To every ocean depression must correspond a similar larger concavity below, and continental elevations must have much greater protuberances answering to them on the under-side. Thus, could we strip off the crust like a hide, and turn it over, we should find the under-side reproducing the upper-side, only with every feature magnified.

This conception may be deemed at first sight strange and wild, yet it certainly affords an easy explanation of one or two rather singular phenomena. It was found during the Indian Survey that the mountain mass of the Himalayahs attracted a plummet much less than it ought to do, and that the cavities which contain the waters of the ocean, instead of causing a diminution of attraction, show an increase. Now it is shown that the protuberances of light material below the former and the concavities filled with the denser substratum below the latter would produce exactly such results. Also the hemisphere of water, which maintains its position in spite of continental attraction, is thus sufficiently accounted for. Again, since the floating crust must sink wherever weighted, and rise wherever material has been removed, we see how vast thicknesses of sediment might be accumulated without much perceptible change of depth, and mountains suffer continual degradation, and yet never be entirely effaced.

Another remarkable argument is derived from the observations in the St. Gothard Tunnel, which show that the rate of increase of internal temperature is slower there than beneath plain countries, and slowest where the mountain is highest. This should not be the case, perceptibly were the earth cooling as a uniform solid. Assuming these rates to be uniform, and allowing for the cold due to the elevation, it is easy to calculate the depths at which any particular temperature would be reached. If there be a molten nucleus its surface should be a surface of uniform temperature. But the depth at which a temperature of fusion can be reached will be found far greater under the mountains than under the plains. Hence, says Mr. Fisher, the solid crust must have protuberances below, answering to the mountains above.

This argument is weighty. It approaches near to

demonstration. If this slower rate of temperature-increase below mountains were satisfactorily made out, and if we could be sure that the rate remained uniform at all depths, the existence of such protuberances would be almost proved. It is difficult to see what other supposition could be made. However, with the wide discrepancies at present experienced in the observations of such temperatures, and with the evidence that exists for a rate depending on the depth, a sceptic is not quite compelled to assent.

No theory of the earth's crust can be complete which does not provide the machinery for earthquakes and volcanoes. Mr. Fisher, for this purpose, supposes his subterranean fluid to contain, in intimate union with itself, vapour in considerable quantities. This vapour is to be retained in the fluid by the superincumbent pressure, as gas is in the liquid of a soda-water bottle, and will, if such pressure be removed, be disengaged from the molten matter as the gas disengages itself when the cork is drawn, though much more slowly, by reason of the viscosity of the fluid. This agrees with the view taken by Prof. Judd in his recent volume on Volcanoes. It will be a novel idea for many of us to imagine the earth like a globular bottle of effervescent liquid, and its crust like ice covering a lake of aerated-water. But such a constitution would account for many of the phenomena of eruptions. The earthquakes which usually herald them, the rise of molten material in a fissure, the existence of permanently liquid lava like that in Kilauea, the quiescence of neighbouring vents, the growth, death, and revival of a volcano, all follow as natural consequences. The difference in the lavas ejected from adjacent craters and the supposed order of succession in the products erupted are also accounted for, but not so satisfactorily. The theory is a very important one, and appears on the whole the most satisfactory that has yet been propounded.

It is natural to suppose that the emission of the vapour from this substratum would tend to produce a contraction of the nucleus. When we consider how far the volume of the ocean exceeds that of the continents it is surprising to be told in the chapter on the Extravasation of Water that this supposition cannot account for them. However it will be found on examination that much depends on the hypothesis. The supposition made is not local emissions of liquid producing cavities, but a general exudation and consequent crumpling of the crust. The analogy is not to the subsidences in Cheshire, where brine has been removed, but to the wrinkled skin of an apple as it dries.

The reader of this volume must bear in mind that most of the numerical results from time to time obtained and used are deductions from assumed data, and not independent truths. Such is a statement which often occurs in the calculations, that the contraction required to produce the existing inequalities of the earth's surface is 0.0005. He must also distinguish real confirmations of the theory such as the deviations in the plumb-line and the slower subterranean temperature-increase in the neighbourhood of mountains, from mere appearances of coincidence in numerical results. The latter are in several cases necessary consequences of identical assumptions. The agency of intruded dykes in producing elevation and compression does not seem altogether a natural one. We may conceive the crust passing down into fluid, but not

so readily that the fluid should pass again into a solid centre. Another formidable difficulty is that a subterranean ocean must be subject to tides, as much as the sea would be though covered by ice. This is passed over somewhat lightly with the suggestion that viscosity may be sufficient to obscure all tidal phenomena. Doubtless, too, other difficulties will start up for which it may not be easy to find a solution. But every theory is sure to present difficulties. Time must show whether they multiply or die away.

One or two points do seem to emerge from this assemblage of calculations as fairly clear, and established on tolerably firm foundations. Such are, that contraction of the earth by cooling is inadequate to the production of its greater inequalities. The earth cannot be a mass quite so homogeneous as on the theory of having cooled from a perfect fluid it is often assumed to be. There must be subterranean irregularities of density. Besides these, the phenomena of volcanoes seems to be explained best, as yet, by the existence of vapours and gases in intimate mixture with the materials below its crust. And a substratum plastic, if not fluid, will account for many facts which are ordinarily very perplexing. But, to quote from a striking quotation made in the volume itself, "Of all known regions of the Universe the most unsafe to reason about is that which is beneath our feet." E. HILL

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Vignettes from Nature

ANXIOUS that popular scientific literature, especially that which deals with the Evolution-doctrine, should be strictly accurate in its facts, I would ask—in no unfriendly spirit—whether Mr. Grant Allen and Mr. Wallace have fully informed themselves upon each of the several positions taken in the paragraph cited with approval by Mr. Wallace (in the last number of NATURE, p. 381) from Mr. Grant Allen's "Vignettes," referring to the dimensions of the largest animals now existing, as compared with those of the faunæ of past epochs.

1. It is asserted that "no known extinct animal was as large as some of our modern Whales." When, some thirty years ago, I visited the so-called "coprolite" pits in the Suffolk crag, I was astonished at the multitude of the ivory-like "ear-bones" of whales found in a certain group of them; which were described by Prof. Owen, and compared with those of existing *Balenidæ*, in his "Fossil Mammals of Great Britain." From the fragments of gigantic ribs and vertebrae which I then saw at Felixstowe, I should certainly suppose the extinct whales they represent (which Prof. Owen regards as of Eocene age) to have been fully as large as those of the present time.

I would ask, further, whether sufficient account has been taken, in the statement just cited, of the most gigantic types of Reptilian Mesozoic life? Any one who has placed himself by the side of the huge bones of the *Cetiosaurus* which form such a conspicuous feature in the Oxford Museum, must, I think, be disposed to regard the animal there represented as having probably at least equalled the whale in bulk, though very likely not in length. And even this colossal reptile must have been far exceeded in dimensions by the *Atlantosaurus montanus* described by Prof. Marsh from the Wealden of Colorado. I would respectfully ask the authors, therefore, whether they are prepared to show that such an estimate is fallacious.

2. Having been led to believe, by all I have seen, heard, and read, that the ordinary bulk of our existing Elephants (I do not

speak of exceptional "Jumbos") was considerably exceeded by that of the Mammoth and Mastodon—the former surpassing them in height (see the comparative measurements given by Prof. Owen, *op. cit.*), and the latter in length of body, I cannot but feel surprised that Mr. Grant Allen should speak of elephants "as having been increasing in size from the earliest epoch of their appearance to the present day"; still more, that Mr. Wallace should endorse the statement. Of course I shall at once bow to the superior knowledge of the latter most distinguished zoologist, when he refers me to trustworthy measurements in support of his position.

3. I can speak with more confidence in regard to the relative size of extinct Sharks, none of which, in the judgment of Mr. Grant Allen and Mr. Wallace, surpassed the forty-foot sharks of the present time. For I have now before me a tooth of a fossil shark (found in one of the before-mentioned "coprolite pits") of pretty regular triangular form, measuring four inches in length, three inches across the base, and seven-eighths of an inch in thickness between its flat surface and the most protuberant part of its convex surface; and I have seen others much larger, the length of some being said to range to six inches. Now when I brought this tooth home, I took an early opportunity of comparing it with the largest teeth of existing sharks that I could find in the British and Hunterian Museums, and found these to be pigmies by comparison. Unless, therefore, I can be referred to some fresh source of information, I must continue to believe (*pace* Mr. Grant Allen and Mr. Wallace) that some of the older sharks were far larger than any of which we have any knowledge at present.

4. Is it clear that *Tridacna* is the largest known Mollusk? I should have thought it exceeded by the gigantic *Ammonitida*, the largest specimens of which are not always to be found in museums; for I have seen one at Redcar (whose diameter I am afraid to state from memory, for fear of exaggeration) so massive that no one had undertaken the task of removing it.

5. No mention is made of *Crustacea*, though I should have thought that important class worthy of notice. I would ask where any existing crustacean types are to be found, that surpass in size the gigantic *Eurypterida* or even the largest *Trilobites*.

6. Of the *Foraminifera*, one of the most important classes in the whole animal kingdom for the share it has taken in the formation of our limestone rocks, I venture to speak with some special knowledge. The largest examples of this group known to us at the present time are the *Oribolites* and *Cyclolypus*. The former is a very widely diffused type, but only under peculiar local circumstances exceeds an inch in diameter, or one-tenth of an inch in thickness; the latter is (so far as I am yet aware) restricted to one locality, and, though attaining the large diameter of 2½ inches, is scarcely thicker than an ordinary card. If these be compared with the massive *Nummulites* and *Oribolites*, of which the vast *Nummulitic* limestones are composed, the advantage will be found clearly on the side of the latter.

But, in conclusion, I think it will be conceded that in estimating the general dimensions of a Fauna, we must take into account not merely the size of its largest animals, but the range of their distribution; and I would ask Mr. Wallace (whose knowledge of this subject no one appreciates more fully than myself) whether this consideration has been duly weighed by him. Our existing colossal land mammals (elephants, giraffes, rhinoceroses, and hippopotamuses) are limited to the tropical and sub-tropical regions of the Old World; while the great American continent is entirely destitute of them. Let this state of things be compared with the former extension of the Mastodon¹ and Mammoth through North America (which had for its own also the gigantic *Bronthotheriidae*), as well as over Europe and Northern Asia; and the nearly equal range of the Rhinoceros and Hippopotamus (some species of all which seem to have lived contemporaneously during the Quaternary Period); whilst at the same time the wide area of South America was tenanted by another Mastodon, as well as by the colossal Sloths. There can be no reason to suppose again that the great *Balenidæ* were less abundant during the later Tertiary and Quaternary epochs, than they were either previously or subsequently. And if the evidence of the abundance of some of the colossal land-Mammals—afforded by the vast accumulation

¹ That the Mastodon, though it appeared much earlier than the Mammoth in the Old World, continued to exist in the New during the Quaternary period, is now, I believe, generally admitted. I myself, at the request of Dr. Warren, examined the contents of the well-preserved specimen obtained by him, and found therein twigs quite fresh enough for the microscopic recognition of their Coniferous structure; and Prof. Asa Gray told me last summer that he could clearly identify them with a well-known existing type.